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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Inclusion of mountain wave-induced cooling for the formation of PSCs over the Antarctic Peninsula in a chemistry–climate model" *by* A. Orr et al.

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General Comments

1. *From the abstract, the introduction and the model description the paper promises to resolve a longstanding problem in chemistry climate models, most results are however presented just for one gridbox and for an example at an altitude and time that is not relevant for the ozone hole. In Figure 5 are more interesting cases which were not addressed.*

   Yes. These concerns have been clarified/addressed in the revised manuscript.

Firstly, the abstract and introduction have been revised to make clear that the paper does not resolve 'a longstanding problem in chemistry climate models' (i.e. the realistic representation of mountain wave-induced PSCs and their attendant ozone-loss chemistry), but rather is a step towards this. This is achieved by emphasising that the chemistry-climate model is only used to perform sensitivity studies focused on the representation of regional PSCs for the Antarctic Peninsula. To this end, the final couple of sentences in the abstract have been revised to read: ***'The parameterisation was used to include the simulation of mountain wave-induced PSCs in the global chemistry-climate configuration of the UM. A subsequent sensitivity study demonstrated that regional PSCs increased by up to 50% during July over the Antarctic Peninsula following the inclusion of the local mountain wave-induced cooling phase.'*** Moreover, two additional sentences have been added to the introduction section which read: ***'However, we only evaluate the sensitivity of PSC simulation in the chemistry-climate model to the inclusion of the mountain wave-induced temperature fluctuations for one regional example, the Antarctic Peninsula. (The contribution of the scheme to global PSCs and ozone chemistry will be fully assessed in a subsequent manuscript.) This is because the main purpose of this study is to assess the ability of the parameterisation to simulate stratospheric temperature fluctuations, which is achieved by using case studies of AIRS measurements to validate high horizontal resolution simulations (using the regional mesoscale configuration of the UM) of mountain wave-induced stratospheric temperature fluctuations above the Antarctic Peninsula (section 3)'.***

Secondly, we have addressed the reviewers concern that the results of Figs. 5, 7 and 8 are just for a single grid point (located at -70 N and -63.75 E). This is a fair comment, as although Fig. 3 shows that this point is representative of the CS3 mountain wave event, it is less representative for CS1 and not at all for CS2. In the revised manuscript we therefore select points individually for each of the three case studies which coincide with the location of the respective mountain wave event. These are located at (67.5 S, 63.75 W) for CS1, (65.0 S, 60.0 W) for CS2, and (70.0 S, 63.75 W) for CS3. Following this, it was evident that while the revised Fig. 5 suggests that the temperature fluctuations computed by the parameterisation scheme are in good agreement with the mesoscale results for CS1 and CS3, the agreement for CS2 is poor. An explanation for the poor agreement for CS2 is that the surface winds in CS2 were significantly different in the climate model compared to the mesoscale model, suggesting poor skill in capturing surface winds by the climate model which impacted the parameterisation response. Section 4 has been modified substantially to reflect these results. Moreover, the sentence in the abstract which stated in the original manuscript that the parameterised temperature fluctuations ***'are in excellent agreement with the mesoscale configuration responses'*** has been revised to say they ***'are in good agreement with the mesoscale configuration responses for two of the three case studies'***.

Thirdly, we disagree with the comment that 'most results are however .... for an example at an altitude and time that is not relevant for the ozone hole'. Here, the reviewer is referring to Figs. 9, 10 and 11 which were for July at an altitude of 21 km. However, we accept the relevance of this month and altitude to ozone depletion was not made clear in the original manuscript. To remedy this, the beginning of the third paragraph of the introduction has been revised to say: ***'The role of PSC particles in polar ozone chemistry is generally well understood. In the winter when there is not enough sunlight in the polar stratosphere to initiate photochemistry, the conversion of reservoir chlorine molecules into chlorine gas takes place on the surface of PSCs. In the spring when the polar stratosphere becomes sunlit, ultra-violet radiation splits the chlorine gas molecules into chlorine atoms, which take part in reactions which destroy ozone (Solomon, 1999).'*** Moreover, an additional sentence has been added to the final paragraph of the introduction which reads: ***'Note also that the***

availability of sunlight at the Peninsula during mid-winter to initiate photochemistry means that ozone depletion is substantial over this region from mid-winter onwards (Roscoe et al., 1997).' Finally, the reviewer should note that, despite the slower pace of ozone loss in the edge region of the vortex (where the Antarctic Peninsula lies), ozone amounts there regularly fall below 120 DU, very comparable to the 100 DU amounts seen in the vortex core, and that the edge region of the vortex contains about half the area of the ozone hole (Roscoe et al. 2012). Hence conditions in winter over the Antarctic Peninsula are of extreme relevance and interest to the ozone hole.

Fourthly, the reviewer makes the comment 'in Figure 5 are more interesting cases which were not addressed'. We reiterate that Fig. 5 (and more generally the three case studies) are purely used to validate/assess the temperature fluctuations computed by the parameterisation scheme. The investigation never had any intention of examining or simulating PSCs for these case studies.

Roscoe, H. K., M. Trainic, W. Feng, M.P. Chipperfield, E.F. Shuckburgh, "The existence of the edge region of the Antarctic stratospheric vortex", J. Geophys. Res. 117, D04301, doi:10.1029/2011JD015940 (2012).

2. *What happens at other gridboxes shown in Figure 11 in the light of the problems shown in Figure 6 (and page 18290, lines 25ff)? Can the approach be generalized also for the Arctic? Is the mountain wave parameterization used only for temperature or also winds and advection of chemical species? Here a lot of clarifications are necessary. To be acceptable major revision is needed.*

   Yes. These concerns have been clarified/addressed in the revised manuscript.

   Firstly, with regard to what happens to other grid boxes, please see our reply to general comments 1 (above).

   Secondly, Fig. 6 has been revised to also show the parameterised cooling phase (which is the field which is passed/coupled to the PSC scheme of the chemistry-climate model).

Thirdly, the parameterisation scheme is (globally) implemented in the climate and chemistry-climate configurations of the UM, and so can be used for the Arctic. This has been made clear in the revised manuscript. For example, we have added an additional sentence to the final paragraph of section 6 which reads: ***'Further future work will also involve evaluating and improving the representation of PSC formation mechanisms in the chemistry-climate model via comparison with MIPAS (Michelson Interferometer for Passive Atmospheric Sounding) PSC observations (Spang et al., 2012), resulting in improved modelling and more reliable projections of both Antarctic ozone hole recovery and Arctic ozone.'***

Fourthly, the parameterisation is only for temperature, and not for winds or advection of chemical species. To make this clear an additional paragraph has been added to the end of section 2.1 which reads: ***'Note that all of the configurations of the UM parameterise the vertical divergence of mountain wave-induced momentum flux (i.e. orographic gravity wave drag), which influences the atmospheric circulation. This is dealt with by the orographic gravity wave drag scheme of Webster et al. (2003), which should not be confused with the mountain wave-induced temperature fluctuation scheme of Dean et al. (2007), described below. '***

Specific comments

1. *Page 18278, line 24: This important finding is misleading here since it is confined to a special case not relevant for polar chemistry (see section 5).*

   Yes. We have revised this line (situated in the opening paragraph of section 1), which now reads: ***'Gravity waves generated by stratified flow passing over***

   orography (mountain waves) that propagate into the stratosphere can play a role in the formation of polar stratospheric clouds (PSCs).'

2. *Page 18279, line 15: It would be interesting to see results on this.*

   The primary objective of this work is to investigate/validate the representation of mountain wave-induced temperature fluctuations by the parameterisation scheme. See reply to major comment 1 (above).

3. *Page 18283, line 24: On the Peninsula or global?*

   Yes, the parameterisation scheme is implemented globally. To make this clear, we have revised one of the sentences in section 2.2 which describes the parameterisation to read: ***'The parameterisation scheme is (globally) implemented in the climate and chemistry-climate configurations of the UM.'***

4. *Page 18285, line 7: Are these parameters specific for the Antarctic Peninsula or are they used for other mountain ranges too in the climate model?*

   Yes, these parameters are specific for the Antarctic Peninsula. To make this clear, we have revised one of the sentences in section 2.2 to read: ***'Following an initial sensitivity study (specific to the Antarctic Peninsula) to optimise the performance of the scheme, their values were set to .... '***

5. *Page 18286, line 18: Forecast from which model? A weather forecast model as on page 18283 (line 5) or the nudged climate model? Please clarify here, not one page later.*

   Yes, this has been clarified. One of the sentences in section 2.4 has been revised to read: ***'These events were simulated by running the nested mesoscale model for a 48 h period, driven by output from the global model initialised on 5 August 2011 at 12:00 UTC for CS1, 1 August 2010 at 00:00 UTC for CS2, and 13 July 2010 at 00:00 UTC for CS3.'*** The use of the terms 'nested mesoscale model' and 'global model' are now clearly explained in section 2.1, which now states: ***'The mesoscale model is nested within a global version of the model at a horizontal resolution of N512 (1024 x 769 grid points, or 0.352 x 0.234 degrees) and 70 vertical levels (reaching up to 80 km) that is used (following a 3 h spin-up) to initiate and provide boundary conditions for the simulation.'*** Additionally, section 2.1 now states (with the addition of one extra sentence): ***'The UM is a numerical modelling system based on non-hydrostatic dynamics which can be run with varying configurations, including for this study as a regional mesoscale model, a global climate model, and a global chemistry-climate model. Hereafter, these models are referred to as the mesoscale, climate, and chemistry-climate models, respectively.'***

6. *Page 18287, line 4: This paragraph should go at least partially to section 3, here it interrupts the flow.*

   We disagree with this suggestion. Section 3 is a results section, and as such should not contain any methodology.

7. *Page 18288, line 12: Where? Peninsula only?*

   Yes, we have clarified this. In the revised manuscript it is now clearly defined in section 2.1 that the chemistry-climate model is a global model (see reply to specific comments 5), but that we are focusing on one regional example, the Antarctic Peninsula. However, to avoid confusion we have added an additional sentence to section 2.4 which states: ***'For this part of the study we again concentrate on results for the Antarctic Peninsula, focusing on the month of July.'*** See also reply to general comments 1 (above).

8. *Page 18290, line 18: More quantitative information would be good here.*

   Figure 1 in the supplementary material demonstrates clearly that the climate model at N96 resolution completely fails to resolve any temperature fluctuations.

   To clarify this in the revised manuscript we now state: ***'Note that the climate model simulations of the three case studies were repeated at a higher N96 resolution (192 x 145 grid points, or 1.875 x 1.25 degrees), which also completely failed to resolve any temperature fluctuations over the Peninsula (not shown).'***

9. *Page 18291, line 5f: In N96 the smoothing should not be so severe. Can the N96 results be shown too? Line 20: I don't believe that.*

   Firstly, the optimisation of the mountain wave parameterisation scheme for the Antarctic Peninsula was only done for N48 resolution. We therefore do not have equivalent results at N96 resolution which we can show.

   Secondly, we have removed 'line 20' which stated ***'If the mesoscale model response would be spatially averaged the amplitudes would be in good agreement.'*** The analysis presented in Fig. 5 of the manuscript (i.e. assessing the derived temperature fluctuation for different climate model grid boxes around the Antarctic Peninsula) is a more quantitative comparison than simply averaging over a long transect as the negative (cooling) and positive (warming) phases largely cancel each other out. Therefore whether the amplitudes were in good agreement when averaged over a long transect is not important. We have added an additional sentence in section 4 which includes words to this affect.

10. *Page 18291, line 27ff: The selected time and altitude is not interesting for the ozone hole formation even if there are some effects on PSCs. Is it selected because of AIRS (Figure 1)? It is also odd that in Figures 5 and 6 there is a warming at this altitude due to the parameterization. An analysis in August and September at 18km would be much more relevant for the ozone hole formation. It is odd that the PSCs increase upstream of the mountain range (Figs 9 and 11).*

    See reply to general comments 1 (above).

11. *Page 18293, line 11: In the example most of the Peninsula is still in polar night so ozone loss and radiative heating effects should be marginal (please check and quantify). Line 22: Why is there no effect in August to October? Here (re-)formation of PSCs at the Peninsula would be important. More details please.*

    Yes, this has been clarified.

    We have removed the line ***'Note that significant differences in PSC surface area density were also evident in June, but not in August and September (not shown).'*** from the revised manuscript, which was a throwaway line at best. We stress again that the aim of this study was to only show the sensitivity of PSCs to the inclusion of parameterised mountain wave-induced cooling, and this is demonstrated adequately for the July example.

    Nevertheless, the reason why there is a much smaller effect in August and September could be because forming PSCs earlier in the winter dehydrates the stratosphere, making it harder for PSCs to form in late-winter and early-spring. This will be the focus of future investigation.

12. *Page 18295, line 28ff: The beginning of this paragraph would better fit into the introduction. The end needs clarifications, e.g. where exactly the parameterization is applied in the chemistry-climate model.*

    We disagree that the beginning of this paragraph would better fit into the introduction. Here, we discuss that other biases could affect the ability of chemistry-climate models to realistically represent PSCs, which is entirely suitable for the discussion section. We further don't believe that the end of this paragraph needs further clarification. For example, it is clearly explained in the methodology section that the parameterisation is implemented in a global chemistry-climate model and that the wave-induced cold phases is coupled to the PSC scheme. Moreover, we reiterate this in section 6 by saying: ***'Subsequently, we assessed and characterised the localised impact of the parameterised temperature fluc-***

    tuations in a comprehensive chemistry-climate model. It was found that adding the wave-induced cooling phase to the resolved temperature ....'

13. *Page 18305, figure caption: What is wind magnitude? The figure is difficult to read.*

    Yes, this change has been made. This figure has been revised so as to make it more readable. The term 'wind magnitude' in the figure caption has been changed to 'wind speed'.

Technical corrections

1. *Page 18288, line 25: Bad wording, do you mean sea ice fraction, thickness or what?*

   Yes, this has been clarified. We have replaced 'sea ice concentration' with 'sea ice fraction'.

2. *Page 18290, line 11: typo*

   We couldn't find a typo on this line.

3. *Page 18294, line 29: There is a word missing or too much in the sentence.*

   Yes, this has been clarified. To make this sentence easier to read we have shortened it. It now simply says ***'However, the comparison also showed that the parameterisation cannot represent the upstream tilt of the phase lines with height, due to it representing the Peninsula by a series of independent sub-grid scale ridges which each launch a mountain wave vertically through the column of air above.'***

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C9552/2014/acpd-14-C9552-2014-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 18277, 2014.

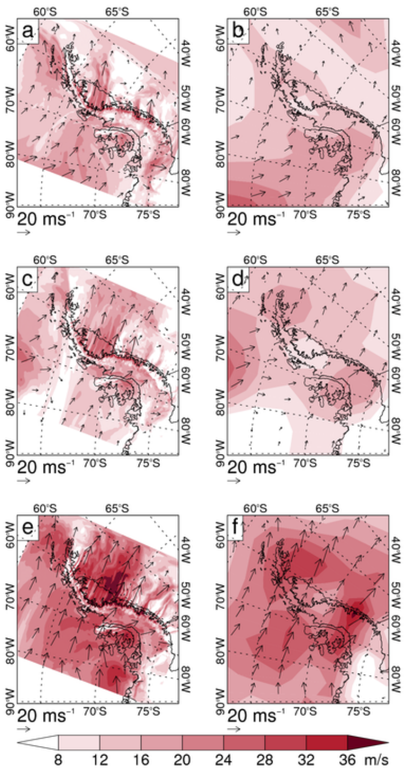
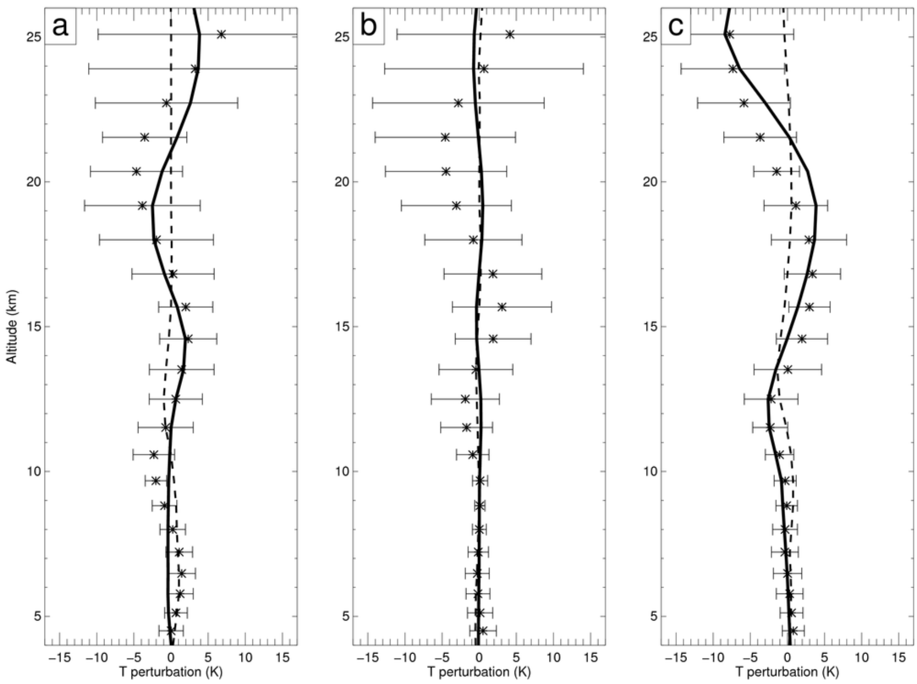


Fig. 1.


a
b
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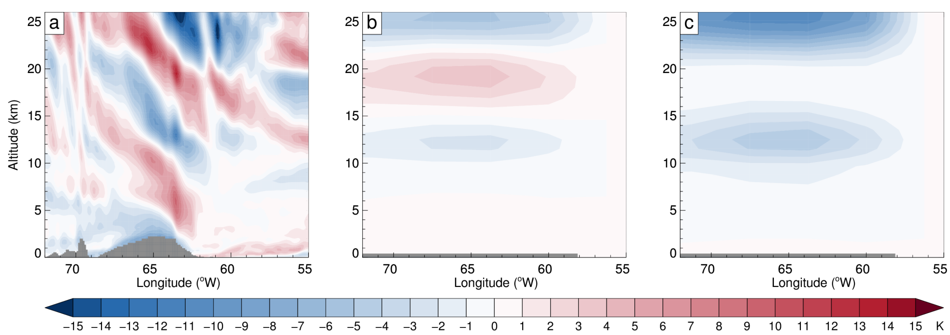
Altitude (km)
T perturbation (K)

Fig. 2.


a
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Altitude (km)
Longitude (°W)
K

Fig. 3.

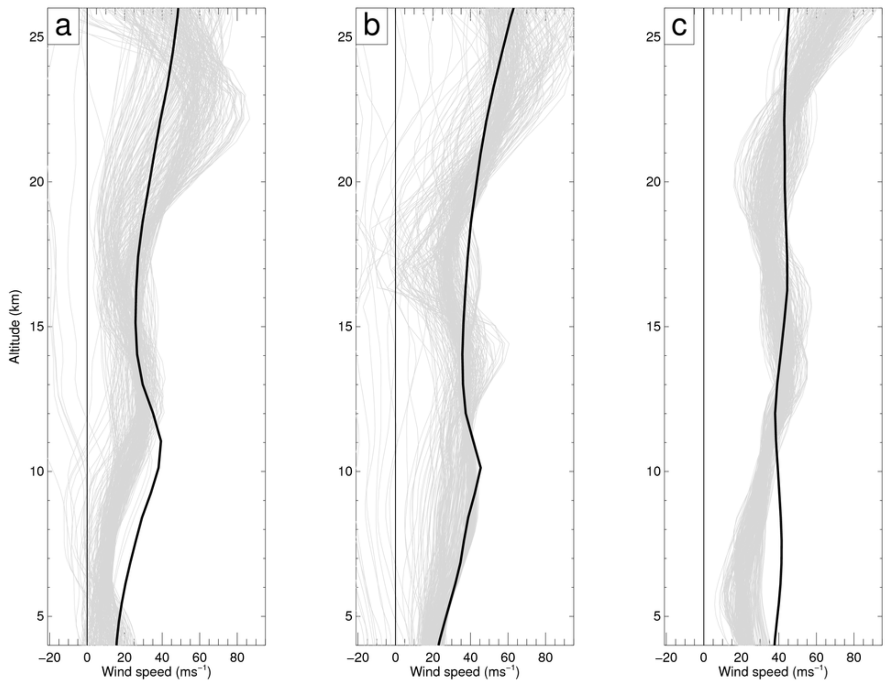

Fig. 4.

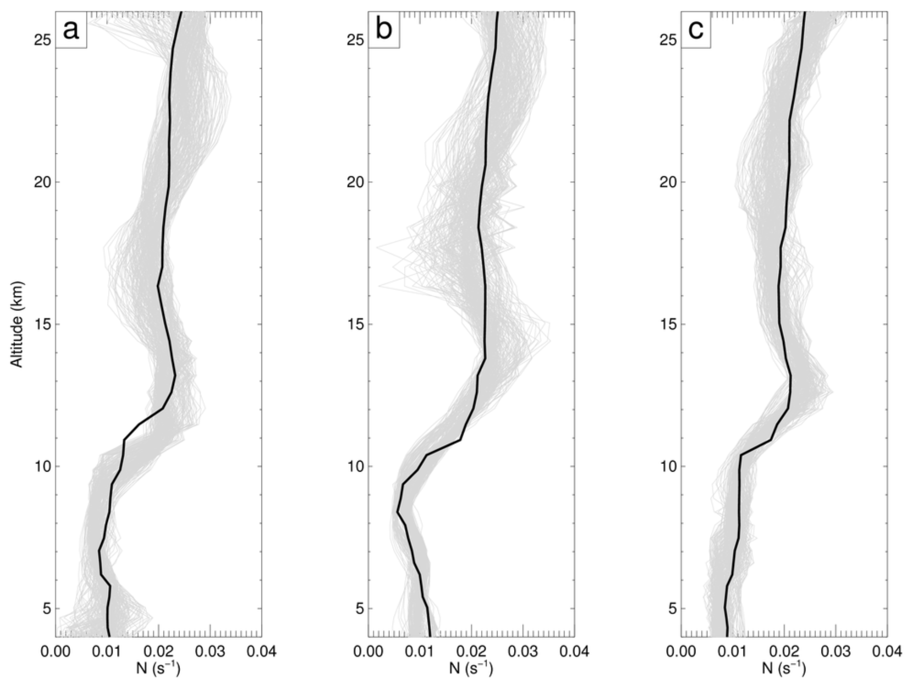

Fig. 5.